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lived, however, some seven or eight weeks, were active, seemed well and happy (?) and, as far as we know, never ate a mouthful of anything during the entire time. I neglected to mention that the old snake shed her skin once during her captivity, unfortunately, it was during our absence, and we did not witness the operation.

It certainly seems strange that, with so much fasting, they none of them should look thin and poor, but should apparently grow and increase when having consumed nothing.

MRS. W. A. KELLERMAN.

Columbus, O., Jan. 9.

Geographical Variation in Birds.

IN your issue of Jan. 6 there appears a communication entitled "Geographical Variation in Birds," containing several remarkable statements, two of which I would like to correct. In speaking of "desert coloration," this writer says: "If the scorching sun of the desert regions will bleach out one species, why will it not do the same for another? The plea of adaptation of coloration for protection cannot be urged here." Such a conclusion does not follow, and the plea of protective coloration might reasonably be made, because the "bleaching" of which he speaks did not take place during one summer, but is the result of natural selection for an unknown number of generations, and, while in some species this protective coloration has proved beneficial, it does not follow that in other species with different habits natural selection would work along the same lines.

But this is only a slight error compared to the following astonishing paragraph, which I quote in full, the italics being my own: —

"Not only are colors affected, but size as well, by geographical position. This is probably more marked north and south than east and west. *And yet the variation in size alone is not sufficient for a sub-specific division.* It is not at all strange that those individuals of a migratory species which push farthest north should possess stronger bones and muscles and so be larger than those which were not able to fly so far. It would seem natural that the constant recurrence of such a difference would tend in time to form a race peculiar enough to be recognized as a sub-species. *But it has not proven true thus far in the history of the world,* and why should there be any change under the same conditions?"

If the above quotation means anything, it is that the author believes increase in size to be more or less general and due to the longer migrations of originally stronger individuals, and yet that this process of selection has not up to this time produced even a tenable sub-species! Considering these two unique ideas in reverse order, let us see whether there are not some species, or at least sub-species, based solely on an increase or decrease in size. Hastily running over the list of North American birds, we find the following interesting facts: *Troglodytes alascensis* is accepted as a different species from *T. hiemalis*, but the variation is only in the size. *Accipiter velox* differs from *A. cooperi* practically in size only. *Totanus melanoleucus* and *T. flaripes* are described by Coues as "precisely the same" except for size. And, lastly, *Rallus virginianus* is "a perfect miniature" of *R. elegans*, being about forty per cent smaller.

In addition to these species, we find there are at least sixteen sub-species which differ from the original stock only in size. About half of these vary from east to west, the others north and south. Furthermore, as Dr. Coues so truly says, many American representatives of European species are "larger and better birds" than their foreign relatives, but we will not include them here, because there are generally some slight differences in coloration as well. So much for the existence of sub-specific variation in size; now, as to the idea that this increase is due to migration. If it is so, how will we account for the cases already given in the genera *Accipiter* and *Totanus*, where the differing species have practically a co-extensive range; or for the even more difficult case of *Rallus*, in which the smaller species is a much more northern bird? But the best illustration to show the fallacy in both ideas is *Dryobates villosus*. Here we have a widely distributed bird, a resident, not a migratory species, which has two accepted

sub-species based solely on variation in size: the northern form, *D. v. leucomelas*, larger than normal, and the southern form, *D. v. audubonii*, smaller. How can this be accounted for on the proposed "migration" theory? To sum up the whole matter, it is probable that northern birds will average larger as a rule, especially in resident species, as they are thus better fitted to stand the severity of the climate and the other difficulties of boreal existence. Furthermore, it can hardly be denied that variation in size is in a number of cases not only sufficient to denote a sub-species, but, occasionally, where the connecting links have disappeared, to form what is universally considered a distinct species.

HUBERT LYMAN CLARK.

Pittsburgh, Pa., Jan. 13.

Pseudoaurora Not Shadows.

THE explanation of the phenomena reported by me in *Science*, issue of the 16th of December, is altogether too common an observation to leave any doubt of its failure to clear up the mystery. My calling has made me very familiar with all of the "shadows cast upon the fog by projecting arms or objects in the beam from the light," as "seen at any time when there is smoke, light fog, or mist."

The phenomena which I described was entirely new to me, and apparently distinct from shadows of any kind, consisting of pencils of light radiating upward from a dark arc, the centre of which was somewhat east of north, the pencils constantly changing in length, and having an apparent movement laterally precisely like those of the ordinary northern lights while I remained standing still. The characteristic coloration of the pencils was unmistakable, but not as distinct as I have sometimes seen it. I have seen the "shadows" so often under similar circumstances of smoke, fog, and mist, that I should scarcely have noticed the matter but for the dark arc with its superimposed luminous arch and the radiations described. But I spent considerable time, in making the different observations mentioned before, and took in the familiar shadows that impress the mind of Professor Hazen so strongly.

P. L. HATCH.
Anacortes, Washington.

Natural Selection at Fault.

WE are generally told by orthodox Darwinians that both the structure and the actions of animals are to a great extent dependent upon natural selection. Any organ, or any habit which is not advantageous to its species will be, it is said, promptly suppressed as a possible danger, or at least, an unremunerative demand.

Yet there are a few cases of habits which seem to have been acquired or maintained in flat contradiction to this doctrine. Every one knows that the *Felidae*, from the Bengal tiger down to our domestic mouser, when they have seized a prey do not at once kill and devour it, but either torment it or at least sit and watch it for some time before administering the fatal bite or blow. The consequence is that the victim sometimes escapes, as we all have witnessed, when pussy is playing with a mouse. One instance at least is on record where a man, struck down by a tiger, quietly drew a dagger and stabbed the assailant to the heart. This could not have been done with a beast of prey of the canine or ursine family, as they do not allow time for devising and executing such a manoeuvre. Hence we see that the peculiar conduct of the cats is disadvantageous to themselves, and we ask why it has not been abandoned. Certainly any cat which should at once devour any mouse or bird which it had caught would, in times of scarcity, have a decided advantage over its fellow-cats.

Similarly injudicious is the conduct of the domestic hen. As soon as she has laid an egg she at once announces the fact to all whom it may concern by her well-known cackling. What benefit is this outcry to herself or to her species? On the contrary, the outcry is heard by animals which are given to stealing eggs and is understood by monkeys, if we may accept the evidence of La Vaillant. There again, therefore, we have a line of conduct quite contrary to what natural selection would determine.